



Psychology versus Praxeology

by Robert Murphy

[Posted October 21, 2003]

[In a recent article](#), I laid out the Misesian approach of *praxeology*, or the science of human action. Contrary to the mainstream positivist position, in which all economic theories must lead to falsifiable predictions

that can be tested, Ludwig von Mises believed that valid economic theorems must be deducible from the axiom, "Humans act." [\[1\]](#)

Many readers sent me email, challenging the Misesian view. One major concern was whether psychology should avail itself of the experimental method. After all, psychology too deals with human beings, and in particular with their mental states and dispositions. So does the Austrian believe that the method of the natural sciences—in other words, the formulation of a hypothesis that is then subjected to experimental verification or refutation—is as inappropriate in psychology as it is in economics?

In response, I pointed out that Mises was quite clear on the dividing line between psychology and praxeology: Psychology deals with theories to explain *why* people choose certain ends, or *how* people will act in certain settings. Praxeology, on the other hand, deals with the logical implications of the fact *that* people have ends and the fact *that* they act to achieve them. Because of this difference, it may be entirely appropriate for psychologists to engage in experimental testing of hypotheses, while utterly misguided for economists to similarly ape the method of physics.

I. A Psychological Law

To illustrate the difference, let us compare two "laws" from the respective disciplines. Psychologists have conducted many experiments confirming the apparent truth of the "bystander effect," which states that people will be less likely to help someone (a stranded motorist, for example) if there are a large number of other bystanders. The intuition for this apparent law is clear: A person who drives by a stranded motorist in a large city will think, "Somebody else, more suitable than me, will stop and help that person." On the contrary, a person who drives by a stranded motorist in a rural area will realize that he is the only one who can help. Paradoxically, it is possible that a person is more likely to be helped if his car breaks down in a relatively deserted area.

Although the bystander effect sounds plausible, and indeed seems consistent with our personal experiences, is it a scientifically validated theory? Well, the only way to test this is to set up a controlled experiment. We can make no *a priori* deductions about the validity of the bystander effect, since we have no self-evident axioms that would lead to its deduction. It's not enough to test the average response time in a big city versus a rural area, because we are not dealing with the same population. (It might be that most people in a big city are just mean, for example.)

Therefore psychologists have conducted experiments in which the subject is placed in a room by himself, and talks over an intercom to other "subjects" (some of whom are really confederates). During the conversation, one of the confederates casually mentions that she is prone to seizures. Then the confederate (whom the actual subject believes to be a test subject just like himself) fakes a seizure over the intercom. The psychologists then observe whether the real subject will get up and leave the room, seeking help for the seizure victim. And lo and behold, through repeated trials psychologists have found that subjects are less likely to leave their room and seek help, the greater the number of other people involved in the intercom conversation. In particular, if the subject is the only person talking to the victim when the "seizure" occurs, then the subject will just about always get up and seek help. But if he is only one of a dozen people conversing on the intercom, he will be much less likely to get up.

Of course, even this experimental confirmation does not *prove* the universal truth of the bystander effect. It could be that, despite their best efforts, the psychologists did not really pick a representative sample of test subjects. Moreover, even if the bystander effect is indeed a fact for the current population of humans, there is nothing to prevent the emergence in one hundred years of a new breed of humans who, whether through culture or genetics, do not obey the bystander effect. Just like any "law" from the natural sciences, the "laws" of psychology (insofar as they are validated by the experimental method) are only tentative.

II. An Economic (Praxeological) Law

In contrast, let us analyze a typical economic law: If the government runs a deficit, then interest rates will be higher than they otherwise would have been. Now this law too seems commonsensical (just as the bystander effect), but it is more than that: Once the economist takes care to precisely specify the definitions of the terms, he or she can actually *prove* the proposition as an exercise in pure logic. There is no reason to go out and "test" whether it is true, because this would miss the point. It would be as nonsensical as "testing" whether the interior angles of a triangle (in Euclidean geometry) add up to 180 degrees.

During the 1980s, many apologists for the Reagan Administration's profligate spending used statistics in an attempt to refute this praxeological law. They showed that, contrary to those worrying about the "crowding out effect," the huge deficits in the 1980s were not correlated with huge increases in interest rates. Thus it seemed that the economy could have its savings and eat them too; record-breaking deficits weren't bad after all!

Murray Rothbard explained the absurdity of this argument. Statistics, he pointed out, cannot trump logic. If the government runs a deficit (i.e. spends more money than it receives in tax and other revenues), then it necessarily reduces the amount of savings available to the private sector. To put it another way, if the government floods the market with billions of dollars in new bonds, the supply of bonds is necessarily higher than it otherwise would have been, meaning the market-clearing interest rate (the inverse of the price of bonds) is necessarily higher than it otherwise would have been.

What happened during the 1980s, therefore, is that (for whatever reasons) ^[2] interest rates would have fallen tremendously from their heights in the 1970s. But because Reagan ran huge deficits at the same time, interest rates were prevented from falling

as much as they otherwise would have. Without an antecedent theory to relate savings, deficits, and interest rates, the statistician who looks at economic data would be at a loss to come up with anything but shifting (and often spurious) patterns. In particular, there is no way to "test" the rival interpretations of the Reagan era, because economists cannot set up the same "initial conditions" and run the experiment again, this time with a balanced budget.

In conclusion, the Austrian methodological position remains solid. It is true that other disciplines dealing with human beings, such as psychology, can make use of the experimental method. However, insofar as one believes (as Mises did) that economics proper is a subset of praxeology, then it is clear that all economic theorems must be logically deduced from the action axiom. From the Austrian perspective, to copy the methods of the physicists is to entirely misconceive the nature and purpose of economic theory.



Robert Murphy has been a summer fellow at the Mises Institute and now teaches economics at Hillsdale College. robert_p_murphy@yahoo.com. See the [Study Guide on money](#). See the [Murphy Archive](#). See also the [Study Guide on Method](#).

^[1] In addition to the self-evident action axiom, some economic theorems also require subsidiary assumptions such as "labor is onerous" and "labor is scarce relative to land."

^[2] Of course, a Reagan apologist might argue that his policies were the reason for the tendency for lower interest rates, and that a balanced budget (coinciding with the 1981 tax cuts) was politically impossible. That may very well be. But I am making the point that the Reagan record did not *disprove* the "crowding out effect."

Psychology versus Praxeology

by Robert Murphy

[Posted October 21, 2003]

[In a recent article](#), I laid out the Misesian approach of *praxeology*, or the science of human action. Contrary to the mainstream positivist position, in which all economic theories must lead to falsifiable predictions that can be tested, Ludwig von Mises believed that valid economic theorems must be deducible from the axiom, "Humans act." ^[1]

Many readers sent me email, challenging the Misesian view. One major concern was whether psychology should avail itself of the experimental method. After all, psychology too deals with human beings, and in particular with their mental states and dispositions. So does the Austrian believe that the method of the natural sciences—in

other words, the formulation of a hypothesis that is then subjected to experimental verification or refutation—is as inappropriate in psychology as it is in economics?

In response, I pointed out that Mises was quite clear on the dividing line between psychology and praxeology: Psychology deals with theories to explain *why* people choose certain ends, or *how* people will act in certain settings. Praxeology, on the other hand, deals with the logical implications of the fact *that* people have ends and the fact *that* they act to achieve them. Because of this difference, it may be entirely appropriate for psychologists to engage in experimental testing of hypotheses, while utterly misguided for economists to similarly ape the method of physics.

I. A Psychological Law

To illustrate the difference, let us compare two "laws" from the respective disciplines. Psychologists have conducted many experiments confirming the apparent truth of the "bystander effect," which states that people will be less likely to help someone (a stranded motorist, for example) if there are a large number of other bystanders. The intuition for this apparent law is clear: A person who drives by a stranded motorist in a large city will think, "Somebody else, more suitable than me, will stop and help that person." On the contrary, a person who drives by a stranded motorist in a rural area will realize that he is the only one who can help. Paradoxically, it is possible that a person is more likely to be helped if his car breaks down in a relatively deserted area.

Although the bystander effect sounds plausible, and indeed seems consistent with our personal experiences, is it a scientifically validated theory? Well, the only way to test this is to set up a controlled experiment. We can make no *a priori* deductions about the validity of the bystander effect, since we have no self-evident axioms that would lead to its deduction. It's not enough to test the average response time in a big city versus a rural area, because we are not dealing with the same population. (It might be that most people in a big city are just mean, for example.)

Therefore psychologists have conducted experiments in which the subject is placed in a room by himself, and talks over an intercom to other "subjects" (some of whom are really confederates). During the conversation, one of the confederates casually mentions that she is prone to seizures. Then the confederate (whom the actual subject believes to be a test subject just like himself) fakes a seizure over the intercom. The psychologists then observe whether the real subject will get up and leave the room, seeking help for the seizure victim. And lo and behold, through repeated trials psychologists have found that subjects are less likely to leave their room and seek help, the greater the number of other people involved in the intercom conversation. In particular, if the subject is the only person talking to the victim when the "seizure" occurs, then the subject will just about always get up and seek help. But if he is only one of a dozen people conversing on the intercom, he will be much less likely to get up.

Of course, even this experimental confirmation does not *prove* the universal truth of the bystander effect. It could be that, despite their best efforts, the psychologists did not really pick a representative sample of test subjects. Moreover, even if the bystander effect is indeed a fact for the current population of humans, there is nothing to prevent the emergence in one hundred years of a new breed of humans who, whether through culture or genetics, do not obey the bystander effect. Just like any

"law" from the natural sciences, the "laws" of psychology (insofar as they are validated by the experimental method) are only tentative.

II. An Economic (Praxeological) Law

In contrast, let us analyze a typical economic law: If the government runs a deficit, then interest rates will be higher than they otherwise would have been. Now this law too seems commonsensical (just as the bystander effect), but it is more than that: Once the economist takes care to precisely specify the definitions of the terms, he or she can actually *prove* the proposition as an exercise in pure logic. There is no reason to go out and "test" whether it is true, because this would miss the point. It would be as nonsensical as "testing" whether the interior angles of a triangle (in Euclidean geometry) add up to 180 degrees.

During the 1980s, many apologists for the Reagan Administration's profligate spending used statistics in an attempt to refute this praxeological law. They showed that, contrary to those worrying about the "crowding out effect," the huge deficits in the 1980s were not correlated with huge increases in interest rates. Thus it seemed that the economy could have its savings and eat them too; record-breaking deficits weren't bad after all!

Murray Rothbard explained the absurdity of this argument. Statistics, he pointed out, cannot trump logic. If the government runs a deficit (i.e. spends more money than it receives in tax and other revenues), then it necessarily reduces the amount of savings available to the private sector. To put it another way, if the government floods the market with billions of dollars in new bonds, the supply of bonds is necessarily higher than it otherwise would have been, meaning the market-clearing interest rate (the inverse of the price of bonds) is necessarily higher than it otherwise would have been.

What happened during the 1980s, therefore, is that (for whatever reasons) ^[2] interest rates would have fallen tremendously from their heights in the 1970s. But because Reagan ran huge deficits at the same time, interest rates were prevented from falling as much as they otherwise would have. Without an antecedent theory to relate savings, deficits, and interest rates, the statistician who looks at economic data would be at a loss to come up with anything but shifting (and often spurious) patterns. In particular, there is no way to "test" the rival interpretations of the Reagan era, because economists cannot set up the same "initial conditions" and run the experiment again, this time with a balanced budget.

In conclusion, the Austrian methodological position remains solid. It is true that other disciplines dealing with human beings, such as psychology, can make use of the experimental method. However, insofar as one believes (as Mises did) that economics proper is a subset of praxeology, then it is clear that all economic theorems must be logically deduced from the action axiom. From the Austrian perspective, to copy the methods of the physicists is to entirely misconceive the nature and purpose of economic theory.

Robert Murphy has been a summer fellow at the Mises Institute and now teaches economics at Hillsdale College. robert_p_murphy@yahoo.com. See the [Study Guide on money](#). See the [Murphy Archive](#). See also the [Study Guide on Method](#).

^[1] In addition to the self-evident action axiom, some economic theorems also require subsidiary assumptions such as "labor is onerous" and "labor is scarce relative to land."

^[2] Of course, a Reagan apologist might argue that his policies were the reason for the tendency for lower interest rates, and that a balanced budget (coinciding with the 1981 tax cuts) was politically impossible. That may very well be. But I am making the point that the Reagan record did not *disprove* the "crowding out effect."